confirm the values derived from former results. The "Hour-glass" or "Kaiser Sea," which is admittedly the most prominent mark on the planet, is a very suitable one for comparisons to find the intervals of rotation. Early in 1869 I saw it with a 4¼-inch refractor as it passed the central part of the disk. On February 2, 1869, it was central at 10h., on February 4 at 11h., and on February 5 at 11h. 30m.

I observed the same object in February of the present year with a 10-inch reflector (power 252), and noted it crossing the planet's central region at the following times :—

1884	h.	m.
February 14	5	55
15	6	35
19	9	5
22	11	4

I have combined my observation of February 4, 1869, with that of February 14, 1884 (as I regard this pair as the best obtained), to ascertain the rotation period. The interval includes 5487d. 18h. 55m. = 474,144,900 seconds. Correcting this for the difference in longitude between Mars and the earth at the two epochs and for defect of illumination (there is no necessity to apply any correction for equation of light, as the apparent diameter of the planet on the dates selected for comparison was about 16", and hence the distances were nearly the same), I find the time of rotation resulting from the discussion of these observations to be

h. m. s.
24 37 22·34 (5349 rotations),

which is in satisfactory agreement with the periods computed by Kaiser, Schmidt, and Proctor from a much longer series of observations. In order to exhibit the small differences between the period now computed and those resulting from some of the best modern determinations, I give the following summary :—

		h.	m.	s.	
	J. H. Mädler ...	24	37	23·8	*Ast. Nach.* 349.
1864,	F. Kaiser	24	37	22·62	*Ast. Nach.* 1468.
1866,	R. Wolf	24	37	22·9	*Ast. Nach.* 1623.
1869,	R. A. Proctor ...	24	37	22·735	*Mon. Not.* vol. xxix. p. 232.
1873,	F. Kaiser	24	37	22·591	*Annalen der Leidener Sternwarte*, vol. iii. p. 80.
1873,	J. F. J. Schmidt	24	37	22·57	*Ast. Nach.* 1965.
1884,	W. F. Denning	24	37	22·34	

It is obvious that Mädler's period of 24h. 37m. 23·8s. is about one second too great. If we take a mean of the other six values (all within 0·6s. of each other) we get

h. m. s.
24 37 22·626

which may be fairly regarded as a very near approximation to the true sidereal rotation period of Mars.

The computations of Kaiser, Schmidt, and Proctor are severally based on very long periods, the comparisons being modern observations with those of either Huyghens or Hooke during the last half of the seventeenth century. It is unfortunate, however, that there is some question as to the correct identification of the spots depicted in some of the ancient drawings. The representations by Hooke on March 2, 1666 (old style), at 12h. 20m. and 12h. 30m., also those by Huyghens in 1659, 1672, and 1683 give a large irregular spot, extending in a north and south direction, which can only be identified as the "Hourglass" or "Kaiser Sea." It would appear, however, that this interpretation is incorrect in certain cases, for the several drawings do not only show disagreements with each other but also when compared with modern observations originate discordances of period, small it is true, but still too large to be attributed to simple errors of observation. No doubt the period which approaches nearest to the truth will become apparent from future observations, though it can hardly admit of definite settlement for many years, inasmuch as the differences between the several times of rotation as above deduced are very insignificant, and must so closely accord with the real period of the planet that the errors such as exist must be allowed to accumulate over a lengthened interval before they will become distinctly manifested. A comparison extending over fifteen years is insufficient for the purpose, for a computed time of rotation, erroneous to the extent of one-tenth of a second, will still, at the termination of such a period, answer to the positions of the markings to within 9 minutes of time. It is to be remarked that Mr. Marth, whose opinion is entitled to great weight, has, for some time, adopted the period of 24h. 37m. 22·626s. for the rotation of Mars. This corresponds to a daily rate of 350°·8922, and forms the basis of his computations in his "Ephemerides for Physical Observations of Mars," annually published in the *Monthly Notices*.

W. F. DENNING

"The Electrical Resistance of the Human Body"

WILL you kindly publish the inclosed from Prof. Dolbear? It furnishes a complete explanation of the discrepancy between his measurements of the resistance of the human body and those which I have recently made. At the same time, as I have pointed out to him, the fact that this resistance may sink below 500 ohms with "soaked skin," even if that be "abnormal," is of the highest physiological importance, and goes far to explain the hitherto mysterious deaths from accidental passage of a current through the body. Most of these, as Prof. Forbes remarked to me, have taken place with alternate, not continuous, current machines.

W. H. STONE

Wandsworth, May 11

College Hill, Mass., April 23, 1884

DEAR SIR,—I have to acknowledge the receipt of your pamphlet "On the Resistance of the Human Body," for which I am obliged. I am glad to know that physiology has some one in its ranks who is interested in that line of work, and who knows what to do in order to settle such vexed questions.

I have also seen in the last *Electrical Review* that has reached me an article on the same matter, in which you refer to me and what has been published concerning some of my work, that needs a little elucidation. In the early days of telephony the experiment was often tried of making the human body part of the circuit in order to see how speech could be transmitted through the body, in the language of those days. Bell wanted to know what the resistance of the body was when in such circumstances, and I measured it from hand to hand when thumbs and fingers grasped the terminals of a wire and found it to vary between 6000 and 15,000 ohms, and wrote to him to that effect, and from that grew out the statement to which you have referred. Now *under such conditions* that work is right, as I have frequently since proved.

It seems to me that when we speak of the resistance of the body or of any body, and do not define what is meant by body, it is fair to assume that the body is the ordinary body under ordinary conditions. If the resistance (the *actual*) of the wire is found to be a thousand ohms by one party and another one files off the rust from the contacts and then finds the resistance less, both parties may be right. Now the skin of individuals is more or less horny in texture, and so has high resistance which soaking may reduce, and the question then properly arises, is the hard skin properly a part of the body? The resistance of a farmer's hand is often twice as great as that of a child's or of a man of sedentary habits, but solely, as I think, because of the thickness and density of his skin. Does not the question resolve itself into this—What is the resistance of a dry hand and the resistance of a soaked hand? What is the resistance of a good conductor and the resistance of a poor conductor? If the poor one is made better in any way, its resistance is correspondingly increased.

If the condition of the body is abnormal, its resistance may also be abnormal. I should call a soaked skin abnormal.

Still it is of the utmost importance that we should know what the resistance is under all conditions, as being the only way to advance in knowledge of the physiological effects of known currents, and I would again express my gratification at your persistent work in this field, and if I can in any way be of service to you I shall be pleased to be employed.

Yours very truly,

To Dr. W. H. Stone A. E. DOLBEAR

Instinct in Birds

MR. GRAVES, who writes on this subject (NATURE, vol. xxix. p. 596), is, I fear, not so accurate an observer as the magpie, for he misquotes the day fixed by the birds for building, and then indicates that the young "mags" are restricted to four in each nest, while the fact is there are often six or seven in a nest. The magpie is too fond of a fresh egg for breakfast to escape the attention of the gamekeeper. I have often seen the greater part of their nest shot down, repaired, and reoccupied by the birds year after year. I know of no bird that begins the work of *nidification* here early in February, nor any that devotes two months to the work. The rook (*Corvus frugilegus*) is the first to

begin, and I have often been told that it does so early on the first Sunday of March, G.M.T.

What I said about the magpie beginning on the first Sunday (old style) was founded partly on report, but mainly on personal observation extending over some years at one breeding-place, where I have often seen them at work for the first time on this particular morning, and on one occasion in another locality on the same day.

This instinct is not confined to any particular tribe or order, but is common, I think, to all wild fowl, and the two instances given by Dr. Rae (vol. xxx. p. 7) of the regularity with which certain birds pass north to their breeding-grounds is precisely the point at issue, as I believe they begin work as soon as they arrive.

Scientific accuracy has not yet been directed to the subject, but there can be no doubt that from some cause, possibly a sharper and better defined division of summer and winter in former ages, all the feathered tribe have inherited an instinct in nest-building and in the time of their arrival at and departure from their breeding-grounds which guides them to a day in many cases without reference to the state of the weather.

WM. BROWN

112, West Regent Street, Glasgow, May 5

Watts's "Inorganic Chemistry"

THE review of my "Inorganic Chemistry" in NATURE of May 1 (p. 3) appears to have been written without much knowledge of the previous history of the work. The reviewer, indeed, writes as if he were criticising an entirely new book, whereas a glance at the preface might have shown him that the volume in question is the first part of the thirteenth edition of Fownes's well-known "Manual of Chemistry," the first edition of which was published in 1844. H. WATTS

151, King Henry's Road, N.W.

The Recent Earthquake

I NOTICE that Mr. Topley, at the conclusion of his communication to you respecting the recent earthquake in Essex, remarks, "but at present we know of no observations in the central parts of Kent, Surrey, or Sussex." I wish therefore to mention, that although I did not myself notice anything in connection therewith, yet an invalid neighbour of mine, lying in bed, distinctly heard a rumbling noise about 9h. 20m., and a moment afterwards perceived some pot plants in front of his window sway to and fro. This is the only incident with which I have been made acquainted. C. L. PRINCE

The Observatory, Crowborough, Sussex, May 3

THE rise in the Essex waters detailed in my letter of last week still continues. Mr. Radforde Sharpe has kindly sent me the following additional heights that the water rises from Messrs. Courtauld and Co.'s well, at Bocking, Braintree, in inches above the surface of the ground :—

May 6	40½ inches	May 9	39½ inches
,, 7	38½ ,,	,, 10	39¾ ,,
,, 8	40 ,,	,, 12	44 ,,

At Colchester Corporation Waterworks Mr. C. Clegg, C.E., reports the rise recorded is still maintained.

Museum, Jermyn Street, S.W. C. E. DE RANCE

W. H. FRANCE.—Any good entomological text-book will give you the information you ask for.

NOTES ON EARTHWORMS

EVER since our great naturalist called attention to the common earthworm, we watch them with entirely different eyes as they creep timidly out on to the lawn or hurry across the gravel walk; as they collect the dead leaves or bits of string and cloth we may have dropped the evening before, or heap up their household refuse outside the entrance to their home.

He long ago pointed out its importance as a geological agent. The surface of the ground would be very different were it not that the earthworm is for ever at work bringing in the decaying vegetation and converting it into mould. And, more than this, the superficial deposits are often modified to a considerable depth by the earthworms, which, carrying the earth mouthful by mouthful, and the gravel stone by stone, invert the order of stratification.

But we must not push this explanation of the origin of the universal surface mould too far. I received one caution from Darwin himself, many years ago when I was talking to him about the manner in which the chalk with which the land was dressed in Kent worked down. He told me to be careful to bear in mind the action of the great Kentish plough as it year by year turned swathe after swathe down the slopes. The result of this plough-down is clearly distinguishable from worm-mould. In his work on earthworms also he refers to another mould-forming agent of more universal operation and hardly less important cumulative effect. My attention was first directed to it by a lecture I heard delivered by Stoppani in Milan many years ago, in which he was explaining the action of the wind in modifying the surface of the earth, and especially in carrying dust, organic and inorganic. Richthofen and Drew have thus explained the origin of the loam that covers half Asia; and Mr. Clement Reid has recently extended the same kind of observation to Great Britain (*Geol. Mag.*, April 1884). Without this addition we can hardly explain how earthworms could find the material for the manufacture of the mould which often fills the interstices of the ruins of a buried city.

We find, commonly, isolated tumps of moss-covered soil, and every gradation from that up to the large patches of mould which hang like little gardens on each sheltered ledge, where the greater part of the material must evidently have been carried from elsewhere and not have been brought up from below; where it is obvious, from the character of the rocks, that the principal part of the mould cannot have been derived so much from them as from the wind-carried fragments of organic and inorganic material and the decomposition of the vegetation that soon began to grow upon it.

But we find also that the earthworms soon appear in such places, and set to work to mix up and modify all this various stuff that has by various agencies been brought together.

As squirrels, burying acorns and nuts in the autumn, have planted many an oak forest and hazel grove, so it is probable that the earthworms plant many of the ash and sycamore trees that we see perched in out-of-the-way corners, where it is difficult to explain how the blown seed can have got covered by mould enough to allow it to germinate. If an overhanging tree drops the seed, or the wind carries it anywhere near the worm's feeding-ground, it is dragged in and planted in leaf mould, and kept moist till spring time. At this time of the year we see clusters of sycamore seedlings growing up together out of the little worm-hills into which they had been dragged heavy end first.

It is therefore interesting to inquire into the various reasons that should make earthworms travel and occupy new ground. Round the margin of an overcrowded colony we should expect them to spread. They cannot live under water, so they have to move away before a flood. It has been stated that "they may live when completely submerged in water for nearly four months" (Romanes reviewing Darwin, NATURE, vol. xxiv. p. 553). But they were killed off by a flood of a couple of days' duration in the Backs of the Colleges at Cambridge in August 1879. Some of them seem to have got on to the paths, which are raised above the surrounding meadows, and there died. Where the greatest number were found dead the ground had been submerged for a longer time. The following carefully recorded observations by the Rev. Henry Russell, of St. John's College, are worth noting :—

"On Sunday, August 3, 1879, our paddock (the inclosed space in which the men play at lawn tennis, in front of the